

vary inversely as the length; so that, in similar ships, it would vary inversely as their displacements. In other words, so far as one element of fighting power is concerned, and that a very important one, the measure of its amount is not the displacement, as Mr. Barnaby now assumes, but the inverse ratio of the displacement.

The fighting power of a ship is thus composed of several diverse and independent elements; and there is nothing approaching to a consensus of professional opinion as to the relative importance of these elements. To assume that they all vary together with the ship's dimensions, or with her weight in tons, is in the highest degree delusive and absurd. The displacement of a ship measures her weight and nothing more. Whether that weight has been effectively and wisely employed in developing a high degree of fighting power, is an entirely independent matter; and one upon which the whole question of fighting efficiency depends. The statement that displacement "always represents power of some kind," merely begs the question. Of course it represents power; but such power is simply that of displacing water. It may represent that and nothing more, or it may represent in addition the possession of great fighting power, or of other desirable qualities. But the possession of such qualities, and the degree in which they will be developed, must depend entirely upon the skill of the designer—an arbitrary personal factor which is not always limited by the cubic feet of displaced volume that are placed at his disposal. Mr. Barnaby himself pointed out in the paper above referred to, that although the *Defence* and *Vanguard* have approximately equal displacements, the latter carried one-half more armour-plating than the former upon three-fourths of the weight of hull; and was so superior in manœuvring capability that she would turn completely around in four and a half minutes, whereas the former vessel required seven minutes to complete a circle. This difference in qualities, and superiority in fighting power, of the *Vanguard* over the *Defence* is absolutely undiscoverable by merely comparing the displacements.

All the comparisons we have seen of the fighting powers of modern ships of war and of our own and foreign navies, have been more or less vitiated by the arbitrary standards that have been selected as the basis of such comparisons. The displacement basis is unreliable and misleading, and furnishes no test whatever of fighting power. It would be extremely difficult to devise any simple standard by which the popular mind may be fairly impressed with the relative powers of our own and foreign navies; while for purposes of exact comparison or of technical discussion no such standard could be regarded as absolute. Before a simple standard or unit of comparison can be framed, which will be satisfactory or useful, naval officers, artillerymen, and constructors require to agree among themselves about the relative importance of the various elements that make up the fighting power of a ship. The defensive values of armour-plating, speed, turning-power, and other protective qualities, and also the offensive values of the gun and torpedo armaments, the ram, speed, &c., require to be separately evaluated and their relative importance determined. If a general agreement could be arrived at as to the relative approximate values of each of these independent elements of offensive and defensive power, an empirical formula might be framed

—such as Mr. Barnaby attempted with insufficient data in 1872—which would fairly represent the gross fighting efficiency of a ship. Till this is done, no rule can possibly be devised which will indicate anything more than the mere opinions of the person who frames it; while often, as in the case of Mr. Barnaby's present displacement basis, the application of the rule may be misleading in a degree which its framer could never have foreseen or intended.

Sir E. J. Reed's letter to the *Times*, and the whole force of the charges contained in it, rests mainly upon the truth of the two assumptions we have considered. The first is that the unarmoured ends of our present ironclads have practically no protective value. This is a point which, as we have said, may be determined once and for all by scientific experiments. The second assumption is that the comparative efficiency of our own ships and those of foreign powers may be approximately measured by merely comparing their displacements. This proposition is unsound, and does not admit of any qualifying corrections short of depriving it of all specific meaning. A scientific standard or unit of comparison which may be fairly applied to the approximate determination of the relative fighting powers of war-ships and navies is greatly to be desired; but before such an one can be framed, the persons who have to use our ships of war and to take them into action, and those who are responsible for their efficient construction, must come to some definite understanding as to what the various elements of fighting power consist of, and what are their relative degrees of importance; and to do so they must call in the aid of Science.

PROFESSOR WILLIAMSON'S DYNAMICS

An Elementary Treatise on Dynamics, containing Applications to Thermodynamics, &c. By Benjamin Williamson, F.R.S., and Francis A. Tarleton, LL.D. (London: Longmans, Green, and Co., 1885.)

PROFESSOR WILLIAMSON is already so well known to the student by his excellent text-books of the Differential, and of the Integral, Calculus, that his appearance in a new field of authorship is sure to excite attention. We accordingly opened the present work with expectations of a very high order. Not, of course, expectations that much novelty of matter could be introduced in an elementary work on a subject which has been thoroughly threshed-out, but that possibly fresh interest and easier assimilability might be given to long-known facts and processes by some novel mode of presentation.

In these expectations we have been disappointed. Either the subject of Dynamics does not admit of treatment superior to that which it has already received, or our authors are not destined to be the pioneers to the possible improvements. Our special reasons for this statement we will give with some detail, but we may begin with some general observations.

From the time in which Jackson, Lloyd, Whewell, and many others, introduced continental methods to the average Honour-man; through the period of Earnshaw, Pratt, Wilson, Tait and Steele, Griffin, Walton, &c., to the Parkinson, Bezant, Routh, &c., of the present day, there has been a plethora of treatises in English on the various parts of elementary Dynamics. Some of these

were robust, and showed considerable vitality, others sickly and short-lived. But, bad or good, among them they have practically exhausted the resources of the subject, so far as the theorems presentable to a beginner are concerned. The only ringing of the changes has been in arrangement, modes of presentation, and proofs.

But from the books of the future, some of which, at least, we may expect to see starting into existence in the present, we naturally, though perhaps vainly, look for something higher and better than this. We now have elementary treatises on the various branches of mathematics required in Dynamics (two, in fact, due to Prof. Williamson himself) so much superior to any that existed even twenty years ago, that we no longer require to have intricate steps of ordinary differentiation or integration introduced into a text-book of that subject. What we require may be summed up in two words, *Foundation* and *Arrangement*. To these must, of course, be added, as a requirement in every scientific treatise, *Consistency*.

The foundations of the subject, in by far the best form in which they have yet been presented, were given by Newton. He expressly states, before proceeding to give his second interpretation of the Third Law of Motion, that (so far) he had been giving principles generally accepted among mathematicians. But we can barely imagine the effort which must have been made by that transcendent genius in extracting such simple and yet all-comprehending statements from the portentous verbiage of even the most able of his precursors. Step by step, in Britain, Newton's system was forsaken; one of his Laws was split up into fragments, another ignored and its place supplied by gratuitous additional Axioms; till at last the monstrous process culminated in the adoption of Duchayla's so-called statical *Proof* of the Parallelogram of Forces. Thus everything was ripe for Thomson and Tait's reintroduction of the grandly simple system of Newton. The results of this step have been alike remarkable and important. These authors also introduced, after the example of Ampère,¹ the notion of separating the science of motion in the abstract (*Kinematics*) from that of motion of matter:—thus lightening the student's work, in Dynamics proper, to at least as great an extent as it is lightened by his previous study of integration and differential equations.

Now, in the book before us, these improvements on the text-books of twenty years ago are only partially adopted. Kinematics is not made a strictly preliminary study, but inserted in detached fragments. The exploded "statical measure" of force haunts us all through the book, sometimes leading to extraordinary results. Thus, opening at p. 30, we find the following passages, in which we have italicised a few words:—

"Acceleration varies as Pressure."

"This equation enables us to determine the velocity generated . . . by a constant force . . . whenever the *pressure* which measures the *force* is known, and also the *weight* of the body."

"Thus a force which is capable of supporting a weight of 112 lbs. is called a force of 112 lbs."

" . . . the same *effort* which would project a small stone to a considerable distance will move a large one but slightly."

¹ Ampère has never, to our knowledge, received the credit due to him for much of his best dynamical work:—e.g. the u, θ equation of central orbits.

Here we see, at a glance, the effects of want of system. Pressure, Force, and Effort are used as completely synonymous and interchangeable terms. Now the first term has a perfectly definite meaning in science (introduced without definition or warning by our authors in § 290 of the book, to the utter bewilderment of the reader fresh from p. 30), and it means something differing from force in exactly the same way as a linear inch differs from a cubic inch. As to the Effort exerted in throwing a stone, we imagine that, if employed at all in scientific language, it would signify properly the work done, not the force applied; the two things differing as a square foot does from a linear foot. Of course our authors do not require to be told this, but why muddle the student by giving him slipshod information which he must *unlearn*, if he is ever to make progress?

On the opposite page (31) we find:—

"If a uniform pressure [force] of 3 lbs. [weight] produce a velocity [speed] of 10 feet [per second] in the first second, find the weight [mass] of the body acted on."

The insertions are ours, made with the view of showing how the question ought to be stated unless there is to be complete confusion of nomenclature.

Since Clerk-Maxwell published his admirable little book on "*Matter and Motion*" there has been left no excuse whatever for a misuse of the word *Velocity*. The adoption of Hamilton's Vector ideas effected an immense improvement in all these elementary matters. Yet we not only find constantly, in the book before us, this confusion of speed and velocity, but something even more grave, of which one example appears in the above extract. This is the use of the word "velocity" in the sense of so many units of length. See, for instance, pp. 28, 29:—

"In what time will a falling body acquire a velocity of 400 feet?"

"If one minute be taken as the unit of time, what should be taken as the value of g ?"

Ans. The velocity per minute acquired in one minute by a falling body."

Now, what on earth is a "velocity of 400 feet" or a "velocity per minute"? To make the first statement intelligible we must add "per (specified unit of time)"; and for "velocity," in the second statement, we must read "velocity in feet"; or, preferably, "speed in feet." The "per unit of time" is already present on *this* occasion.

Under this category we must quote the truly sensational heading of § 19:—

"Relation between Velocity and Space,"

for this is also obviously based upon the above erroneous designation of "velocity" as so many units of length.

In p. 124 we find:—

" . . . time becomes a necessary element when we come to compare the *efficiency* of different agents. For instance, if one agent . . . performs an amount of work in one hour which it requires another five hours to accomplish, the former is said to be five times as efficient." [The italics are in the text.]

But, turn to p. 438, and we read:—a heat-engine being now the "agent":—

" . . . the ratio of the heat converted into work to the heat drawn from the source is called the *efficiency of the engine*." [Again, the italics are in the text.]

It appears from this, as from a former example, that it is necessary to take the same word in two perfectly different meanings according as it is met with in the first (or ordinary dynamical) part of the book, and in the later (or thermodynamical) part. Such at least is the case with the two specially important terms, *Pressure* and *Efficiency*.

It is perhaps hypercritical to call attention to peculiarities of expression which, however they may puzzle him, can scarcely mislead the student. Else we might ask why (p. 8) a point is "*animated* by any number of velocities," or "*subjected* to any number of simultaneous velocities," or why "*additional velocity*" is said in contrast (p. 12) to be "*received*."

We have marked at least a score of places, in addition to those already noticed, in which the same or similar confusion occurs:—and yet we have *read* in all only about a fourth of the book here and there, having glanced over the rest much more hastily. But it is enough to have said, while illustrating our remarks by simple instances, that this is certainly not a book for beginners, nor for any one whose hold of the exact meaning of scientific terms is precarious:—though it may be consulted without danger (scarcely, we should think, with actual pleasure) by a student who, already soundly educated in the *principles* of Dynamics, desires to get a rapid and condensed *résumé* of their development by mathematical methods.

The principle of dual authorship rarely works well in practice. One of the authors of this book invariably speaks of *Centre of mass* (or of *inertia*) of a body, the other as invariably of *Centre of gravity*. And their responsibility has been so thoroughly divided, that *neither* of these terms is defined, so far as we can find (even with the help of the Index), anywhere in the volume. Again, one of the authors seems to have been always on the look-out to put in a little bit of Kinematics wherever he had a chance. And surely a third must have been at work, whose function was to stick in some sections on the *Rotation of a Rigid Body* (p. 92) between the sections on *Circular Orbits* and those on the *Simple Pendulum*.

The extraordinary *Olla podrida* of Schell is one of the authorities mentioned in the *Preface* as having been largely borrowed from. The book would certainly have been very much better had that work been let alone; though no work more richly deserves to be plundered in its turn than does that of Schell, who simply adopts (and too frequently distorts) whatever pleases him.

OUR BOOK SHELF

Les Organismes problématiques des Anciennes Mers. By the Marquis de Saporta. (Paris: Masson, 1884.)

THE views expressed in Saporta and Marion's "*Evolution des Cryptogames*" (reviewed at length in *NATURE*, vol. xxiv. pp. 73, 558) as to the origin of certain markings commonly met with in palæozoic rocks, has led to a long discussion in which many have taken part, the chief champions on either side being Dr. Nathorst, the distinguished Swedish botanist, and the Marquis de Saporta. Dr. Nathorst maintains that they are tracks left by moving or burrowing animals or other inorganic markings, whilst Saporta holds to his original opinion that very many of them are casts of primæval *algæ*, of kinds now extinct. Nearly all of these markings are in bas-relief on the under surfaces of slabs as if they were moulds of prints or im-

pressions traced in the ancient muds, thus at first sight greatly favouring Nathorst's view of their origin. Saporta demonstrates on the other hand that this is a by no means uncommon mode of fossilisation among undoubted plants, and when we reflect on the composition of *algæ*, we shall see that scarcely any other mode of fossilisation among them is possible. A leathery olive green sea-weed lying on an oozy mud would cause an indentation, and if subsequently covered up, would keep the old surface from contact with the fresh mud, until it might, under favourable conditions, have become sufficiently hardened to retain the impression. The sea-weed, as most olive weeds do now, if left in water or fresh mud, would eventually completely dissolve away, leaving no perceptible organic trace of its presence. The cavity thus left would be filled in at last by the overlying mud, and only a cleavage plane would remain, following the contour of the under side of the weed, and marking its former presence. Sometimes, though rarely, the sea-weed might not decay until a cleavage plane had been established around its entire circumference, without leaving the smallest trace of its internal structure, as we often find is the case with far more resisting cryptogamic stems in the older rocks. This Saporta finds is the case with the *Bilobites*, one of the most vexed of all the "*Organismes problématiques*," and he relies with good reason upon their occasional occurrence in this condition and on their reticulated structure to support his contention that they cannot be mere worm tracks or burrows, and that in point of fact they can be naught but the impressions of primordial *algæ*.

J. S. G.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Civilisation and Eyesight

MY attention has only recently been called to a communication from Lord Rayleigh, which appears in *NATURE* for the 12th inst. (p. 340), and on which I crave permission to make a few observations. Lord Rayleigh questions whether the eyes of savages, "*merely as optical instruments*," are greatly superior to our own; and suggests that any superiority which savages possess may depend upon "*attention and practice in the interpretation of minute indications*." He explains that "*the resolving power of an optical instrument is limited by its aperture*," and then proceeds as follows:—

"With a given aperture no perfection of execution will carry the power to resolve double stars, or stripes alternately dark and bright, beyond a certain point, calculable by the laws of optics from the wave-length of light. With sufficient approximation we may say that a double star cannot be fairly resolved unless its components subtend an angle exceeding that subtended by the wave-length of light at a distance equal to the aperture. If we take the aperture of the eye as one-fifth of an inch, and the wave-length of light as 1-40,000th of an inch, this angle is found to be about two minutes; and we are forced to the conclusion that there is no room for the eye of the savage to be much superior in resolving power to those of civilised physicists, whose powers approach at no great distance the theoretical limit as determined by the aperture."

I understand this to mean that optical conditions limit the resolving power of the eye to objects which subtend a visual angle of about two minutes, and that civilised physicists approach this theoretical limit at no great distance.

With great submission to the high authority of Lord Rayleigh, I venture to question whether we have any data from which to draw conclusions with regard to the possible optical powers of the eyes of the human race. We should probably fall into grave error if we were to argue from the reduced eye of Listing,